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remunerative pedagogical position in many branches than is her equally apt brother.

One result of these sexual characteristics is that women more often cling to the older courses in the humanities, the so-called cultural courses. She prefers these studies, not only because there is less opportunity for her in the technical professions, not only because her more usual ambition is to follow that noblest of all vocations, that of the home-maker, but because her tastes and proclivities fit her better for the more esthetic and humanistic studies.

In the coeducational colleges the women now generally exceed the men in number. This slow relative increase of the men, or in some instances actual decrease, has often been attributed to coeducation, the dislike of young men to mingle with young women in the class-room, to be brought into competition with them where they are so often outshone. I doubt this very much. The milksop who resents the rivalry of women, who thinks himself so far superior to them that he is unwilling to be shown his mistake, ought to be tied to an apron string and smothered in his callowness. The real reason is that men are in greater numbers seeking that special training which they do not or can not get in the general college course, while women are seeking that special training which they do get in the humanities. Nor do I think that either are any more swayed by the commercial spirit which so many superficial observers deplore. There are many advantages in coeducation of the sexes, as well as certain disadvantages. The women need that stimulation in self-dependence and originality which the mingling of young men will surely give them, and the men need the greater esthetic cultivation, the broader humanizing outlook, which women fellow students will surely give them. Coeducational colleges will never become women's colleges so long as they offer anything

which men want, and those courses of study which women prefer will always offer that which many, though not all, men will want.

Whatever may be the tendencies of modern higher education, whatever may be the outcome of the various movements now making, who is there that can repress the feeling of exultation and of congratulation in the great achievements, the lofty aims of our colleges and universities? Whether we are to become a nation of scholars or a nation of specialists, I know not, but that we shall become a greater nation, a wiser nation, a more prosperous nation because of the high school, the college and the university is certain.

S. W. WILLISTON.

UNIVERSITY OF CHICAGO.

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*THE LIMITS OF SCIENCE.*

IN moving a vote of thanks a couple of months since, Lord Kelvin said that science positively affirmed creative power and that modern biologists were coming once more to a firm acceptance of a vital principle. These remarks have given rise to an interesting series of letters to the *London Times*, which we reproduce:

When a man of known distinction gives public expression to an opinion it is, of course, received with attention. But its validity will depend, not upon his distinction, but upon the authority which he has achieved in the field to which his opinion relates.

In the domain of physics, to the exploration of which Lord Kelvin has devoted an honored lifetime, he would be a bold man who would cross swords with him. But for dogmatic utterance on biological questions there is no reason to suppose that he is better equipped than any person of average intelligence.

In a recent speech Lord Kelvin has

taken occasion to define with more precision than, perhaps, he has ever done before his view of the possible attitude of scientific inquiry to inorganic nature on the one hand, and to organic on the other. And he has emphasized this in the letter published in your columns to-day.

That view is, as I apprehend, this: In the former, he claims for scientific investigation the utmost freedom; in the latter, scientific thought is 'compelled to accept the idea of creative power.' That transcends the possibilities of scientific investigation. Weismann defines this to be "the attempt to indicate the mechanism through which the phenomena of the world are brought about. When this mechanism ceases science is no longer possible." Lord Kelvin, in effect, wipes out by a stroke of the pen the whole position won for us by Darwin. And in so doing it can hardly be denied that his present position is inconsistent with the principle laid down in his British Association address at Edinburgh in 1871:

"Science is bound by the everlasting law of honor to face fearlessly every problem which can be fairly presented to it. If a probable solution, consistent with the ordinary course of nature, can be found, we must not invoke an abnormal act of creative power." Among the biologists of the present day I apprehend that there are few who are prepared to contend that the Darwinian theory is not so consistent.

It is a common dialectic artifice to state an opponent's position in terms which allow of its being more readily confuted. It is scarcely, however, worthy of Lord Kelvin. What biologist has ever suggested that a fortuitous concourse of atoms 'could make \* \* \* a sprig of moss'? I confess I think that Lord Kelvin's first thoughts were best, and that it is equally absurd to suppose that a crystal could be made in the same way. A fortuitous concourse of

atoms might produce an amorphous mass of matter; but to form a crystal the 'atoms' must be selected and of the same kind, and their concourse is, therefore, not fortuitous. The fact is that the argument from design applies, for what it is worth, as much to a diamond as to a caterpillar. If it is to be rejected in favor of a mechanical explanation in the one case, it is impossible, logically, to maintain it in the other.

Lord Kelvin quotes Liebig as denying that 'grass and flowers \* \* \* grew by mere chemical forces.' If not, it may be asked, by what do they grow? If growth is to be accounted for by a 'vital principle,' this must be capable of quantitative measurement like any other force. If it is physical energy in another form, Liebig's dictum is futile. If not, organisms are not subject to the principle of conservation of energy. Yet this principle was first indicated by Mayer, a biologist.

Physicists, it may be remarked, are not without their own difficulties. But we do not dismiss their conclusions impatiently on that account. Lord Kelvin said that 'ether was absolutely non-atomic; it was absolutely structureless and homogeneous.' He speaks of it as if it were a definite concrete thing like the atmosphere. But we can not picture to our minds how such a medium can possess elasticity, or how it can transmit undulations. The fact is that the ether is a mere mathematical figment, convenient because it satisfies various formulæ. As it is only an intellectual conception, we may invest it with any properties we please. The late Professor Clifford once told me that it was harder than steel. I believe it is now thought to be gelatinous. Anyhow, it is nothing more than a working hypothesis, which some day, like phlogiston, will only have historic interest.

W. T. THISELTON-DYER.

Kew, May 4, 1903.

Many men of science will heartily sympathize with Sir W. T. Thiselton-Dyer's protest against the attack on the Darwinian theory of evolution recently delivered at the University College. But it seems to many of us somewhat astonishing that an institution which professes to stand in the vanguard of scientific work in London, and which possesses its accredited teachers in biology, should open its doors to irresponsible lecturers on 'directivity,' even if they are supported by the doyens of physical science. To these public lectures condemning Darwinism men and women students from all London colleges are invited, and the president of the college congratulates the assembly on the proceedings of the day. I have always understood that the college was absolutely non-sectarian in character, and that religious controversy and theological propagandism were not admitted within its walls. To the founders of the college, Grote, Bentham, Hume, it would have been a painful revelation to find the truth or falsehood of any scientific hypothesis questioned within its walls from the standpoint of theological polemics. I think there is small doubt that the wishes of these founders, that science and scholarship should be treated apart from theological opinions, have been rigorously carried out in the teaching of the many distinguished men who have held chairs in the college. This non-theological attitude has attracted to the college many of our fellow subjects of Buddhist, Mahomedan and Jewish faiths. But will they find the college the same free ground if they see its authorities recognizing a public course of lectures on 'Christian Apologetics'? A faculty of theology making a scholarly study of dogmatics has a totally different footing from an irresponsible association providing a controversial treatment of the basis of modern biological science. The attack is not delivered openly in the organs

where scientific men criticize the foundations of their knowledge, but covertly, with the tacit assumption that the truth in question is hostile to the Christian belief. It can not be too often reiterated that the theory of natural selection has nothing whatever to do with Christianity. Many good Christians accept it on the scientific evidence; many agnostics reject Christianity without being biased by any theory of evolution. If Lord Kelvin wishes to attack Darwinism, let him leave the field of emotional theological belief and descend into the plane where straightforward biological argument meets like argument. Let him examine the facts of heredity, of variation, and of selection, and offer controverting facts. A dozen biological journals would be open to receive his criticisms and meet them with courteous rejoinder. In this way he would be adding to his already immense services to science; he does not forward knowledge when he adds the weight of his name to an anti-Darwinian crusade which does not proceed from the inspiration of science, but from a mistaken notion that man can *a priori* assert what method of conducting the universe is or is not consonant with the Divine dignity.

KARL PEARSON.

HAMPSTEAD, May 7, 1903.

I feel compelled as a physiologist to express my regret that a most distinguished British botanist has thought it necessary to 'cross swords' with the most distinguished of British physicists with reference to a question on which it is desirable that all men of science should be in accord. I shall not inquire whether the views expressed by the director of Kew Gardens in his letter which appeared on May 7 are entertained by biologists generally. My object is to disclaim on the part of my own science, that of physiology, any participation in the opinion that, for the dis-

cussion of biological questions, Lord Kelvin is 'no better equipped than any person of average intelligence.'

The question at issue is how far 'mechanical explanations' can be given of the phenomena of life. The view which for the last half century has been taught by physiologists may be stated as follows: All the processes observed in living organisms are of such kind as to admit of being investigated by the same methods as are used in the investigation of the phenomena of non-living nature—i. e., by measurement of their time and place relations under varying conditions—in other words, by the method of experiment. But, beyond the limit thus stated, we have to do with processes which can not be directly measured or observed. These are, first, the mental processes, whether of man or of animals, in respect of which the experimental psychologist is unable to go beyond the estimation of conditions and effects; and, secondly, the processes of organic evolution by which the organism grows from small beginnings to such form and structure as best fit it for its place in nature. This is the doctrine which was professed by Helmholtz, the founder of modern physiology, as the result of those early investigations which were embodied in his well-known treatise on the 'Erhaltung der Kraft,' in which he demonstrated more clearly than had been done before that the natural laws which had been established in the inorganic world govern no less absolutely the processes of animal and plant life, thus giving the death-blow to the previously prevalent vitalistic doctrine that these operations of life are dominated by laws which are special to themselves. He thereby brought into one the before too widely separated sciences of physiology and physics.

\* It was not until Helmholtz had been engaged for some eight years in building up

the new science of physical physiology that the German physiologist and the English physicist (W. Thomson) came into personal relation with each other at Kreuznach. In one of Helmholtz's letters to his wife he tells of Thomson's 'surpassing acuteness, clearness and versatility,' qualities which impressed him so much that in their intercourse he found himself to be by comparison 'a dullard.' He was evidently wrong. From the botanical point of view, the future Lord Kelvin was no better than 'a person of average intelligence.' But, in all seriousness, it is surely a mistake to suppose that biological problems appeal so little to the intellect that, unless he is an expert, a man of transcendent ability is incapable of dealing with them.

J. BURDON-SANDERSON.

OXFORD, May 9, 1903.

I am quite impenitent at the irrelevant rebuke of the Oxford Regius Professor of Medicine. But he might represent what I wrote with more precision. I did not express so absurd an opinion as that Lord Kelvin "was no better than 'a person of average intelligence.' " Nor do we need in this country a testimonial from Helmholtz to the contrary. What I wrote was: 'For dogmatic utterance on biological questions there is no reason to suppose that he is better equipped than any person of average intelligence.' By 'equipped' I intended that he is not prepared by technical study of the problems on which he pronounces judgment.

Sir J. Burdon-Sanderson concludes by saying: 'It is surely a mistake to suppose that biological problems appeal so little to the intellect that, unless he is an expert, a man of transcendent ability is incapable of dealing with them.' The first clause of this sentence is obviously absurd; the latter is a simple fact. Any one who has taken the trouble to read the admirable volumes

of Darwin's correspondence recently published will easily inform himself that a trained master mind may devote a lifetime to biological problems and yet feel some hesitation in pronouncing decisive judgment upon them. An untrained master mind may hesitate still more. The late Lord John Russell was credited with the capability at a moment's notice of performing the operation for stone or taking command of the Channel fleet. But such versatility is believed to be rare. 'Transcendent ability' will not enable a man without previous training to either paint an Academy picture or read the Hebrew Bible.

In his speech at University College Lord Kelvin is reported to have said: 'Modern biologists were coming once more to a firm acceptance of something, and that was a vital principle.' I deny the fact. And Sir J. Burdon-Sanderson credits Helmholtz with having given 'the death-blow to the previously prevalent vitalistic doctrine that these operations of life are dominated by laws which are special to themselves.' He explains 'these operations' to mean 'the processes of animal and plant life.' Perhaps he will tell us how he reconciles this position with that of Lord Kelvin, on the one hand, and that attributed by Lord Kelvin to Liebig, on the other. The new 'vital principle' is only a resurrection of the old 'vitalistic doctrine.'

One word more. Sir J. Burdon-Sanderson cites Helmholtz for the statement that 'the processes of organic evolution \* \* \* can not be directly measured or observed.' If he will consult recent volumes of the *Philosophical Transactions* or the pages of 'Biometrika' I think he will find reason, in the light of recent research, to disagree with him.

W. T. THISELTON-DYER.

Kew, May 11, 1903.

Tastes differ, of course; but if I were in Lord Kelvin's place I would rather be criticized by Sir William Thiselton-Dyer than defended by Sir John Burdon-Sanderson. His letter in your issue of to-day is in three paragraphs. The first is sugar, the second aloes, and the third sugar again. This sort of sandwich is popular in the nursery; I fancy a man would sooner have his dose undisguised.

After vindicating Lord Kelvin's right to speak with exceptional authority upon a subject widely separated from those to which he has devoted a long and strenuous life, Sir John Burdon-Sanderson goes on to show that he is entirely wrong. Lord Kelvin drew a sharp line across nature, and said that biologists are now engaged in searching for the 'vital principle' which alone can explain the facts of living matter. His mentor asserts the continuity of nature; affirms that the processes applicable on one side of Lord Kelvin's line are equally applicable on the other; and declares it to be the glory of Helmholtz that he demonstrated more clearly than had ever been done before that "the natural laws which had been established in the inorganic world govern no less absolutely the processes of animal and plant life, thus giving the death-blow to the previously prevalent vitalistic doctrine." He does no doubt add that some things, such as mental phenomena in men and animals, are not yet susceptible of explanation; but the same holds good, as Lord Kelvin would be the first to admit, about some of the most important phenomena of non-living matter.

When men of authority thus flatly contradict one another on fundamental questions, it is very hard for the humble inquirer to know what to believe. It becomes all the harder when neither the physicists nor the physiologists can agree among themselves. Sir John Burdon-

Sanderson is evidently not at one with Sir William Thiselton-Dyer, though he reluctantly supports the main contention of the latter. Lord Kelvin says that the ether is absolutely non-atomic, absolutely structureless, and homogeneous. Professor Osborne Reynolds announced not long ago, as the result of the latest investigations, that the ether is atomic or molecular in structure, gave the size of the molecules, calculated their mean free path, and told us that the ether is 500 times as dense as gold, that its mean pressure is 750,000 tons to the square inch, and so forth.

'Whom shall my soul believe?' is the question of the poet, which is echoed by

Your obedient servant,

QUE SCAIS-JE?

LONDON, May 11, 1903.

I suppose I ought to bow my neck to the rod now that it is wielded judicially by the editor of the *Times*. I feel no inclination to do so. Nevertheless, I hope I may be permitted to point out that 'directive power' is, as a matter of fact, 'the stroke of the pen' by which 'Lord Kelvin, in effect, wipes out \* \* \* the whole position won for us by Darwin.'

It is no use mincing matters. Students of the Darwinian theory must be permitted to know the strength and weakness of their dialectic position. What that theory did was to complete a mechanical theory of the universe by including in it the organic world.

The attempt to introduce a directive force into the Darwinian theory is no new thing. It is, of course, only creative power in disguise. The most notable are those of Nägeli in Germany, and Asa Gray and Cope in America. Weismann has generalized them as an attempt to set up a 'phyletic vital force,' and he points out that if we accept anything of the kind 'we should at once cut ourselves off from all

possible mechanical explanation of organic nature.'

I can hardly suppose that Lord Kelvin was not perfectly aware of this.

May I further add that the 'world of spirit to which the world of matter is altogether subordinate,' to which Dr. Alfred Wallace would introduce us, is not, so far as I know, a subject which biologists find themselves in a position to 'investigate'? The 'ether' seems sufficiently perplexing.

W. T. THISELTON-DYER.

Kew, May 13, 1903.

It seems to me that, were the discussion excited by Lord Kelvin's statements to the Christian Association at University College allowed to close in its present phase, the public would be misled and injustice done to both Lord Kelvin and his critics. I therefore beg you to allow me to point out what appear to me to be the significant features of the matter under discussion.

Lord Kelvin, whose eminence as a physicist gives a special interest to his opinion upon any subject, made at University College, or in his subsequent letter to you, the following statements:

1. That 'fortuitous concourse of atoms' is not an inappropriate description of the formation of a crystal.

2. That 'fortuitous concourse of atoms' is utterly absurd in respect to the coming into existence, or the growth, or the continuation of the molecular combinations presented in the bodies of living things.

3. That, though inorganic phenomena do not do so, yet the phenomena of such living things as a sprig of moss, a microbe, a living animal—looked at and considered as matters of scientific investigation—compel us to conclude that there is scientific reason for believing in the existence of a creative and directive power.

4. That modern biologists are coming once more to a firm acceptance of something, and that is—a vital principle.

In your article on the discussion which has followed these statements you declare that this (the opinions I have quoted above) is 'a momentous conclusion,' and that it is a vital point in the relation of science to religion.

I do not agree with that view of the matter, although I find Lord Kelvin's statements full of interest. So far as I have been able to ascertain, after many years in which these matters have engaged my attention, there is no relation, in the sense of a connection or influence, between science and religion. There is, it is true, often an antagonistic relation between exponents of science and exponents of religion when the latter illegitimately misrepresent or deny the conclusions of scientific research or try to prevent its being carried on, or, again, when the former presume, by magnifying the extremely limited conclusions of science, to deal in a destructive spirit with the very existence of those beliefs and hopes which are called 'religion.' Setting aside such excusable and purely personal collisions between rival claimants for authority and power, it appears to me that science proceeds on its path without any contact with religion, and that religion has not, in its essential qualities, anything to hope for, or to fear, from science.

The whole order of nature, including living and lifeless matter—man, animal and gas—is a network of mechanism the main features and many details of which have been made more or less obvious to the wondering intelligence of mankind by the labor and ingenuity of scientific investigators. But no sane man has ever pretended, since science became a definite body of doctrine, that we know, or ever can hope to know or conceive of the possibility of knowing, whence this mechanism has come, why it is there, whither it is going, and what there may or may not be

beyond and beside it which our senses are incapable of appreciating. These things are not 'explained' by science, and never can be.

Lord Kelvin speaks of a 'fortuitous concourse of atoms,' but I must confess that I am quite unable to apprehend what he means by that phrase in the connection in which he uses it. It seems to me impossible that by 'fortuitous' he can mean something which is not determined by natural cause and therefore is not part of the order of nature. When an ordinary man speaks of a concourse having arisen 'by chance' or 'fortuitously,' he means merely that the determining conditions which have led by natural causation to its occurrence were not known to him beforehand; he does not mean to assert that it has arisen without the operation of such determining conditions; and I am quite unable to understand how it can be maintained that 'the concourse of atoms' forming a crystal, or even a lump of mud, is in any philosophic sense more correctly described as 'fortuitous' than is the concourse of atoms which has given rise to a sprig of moss or an animal. It would be a matter of real interest to many of your readers if Lord Kelvin would explain more precisely what he means by the distinction which he has, somewhat dogmatically, laid down between the formation of a crystal as 'fortuitous' and the formation of an organism as due to 'creative and directive purpose.'

I am not misrepresenting what Lord Kelvin has said on this subject when I say that he seems to have formed the conception of a creator who first of all, without care or foresight, has produced what we call 'matter,' with its necessary properties, and allowed it to aggregate and crystallize as a painter might allow his pigments to run and intermingle on his palette; and then, as a second effort, has

brought some of these elements together with 'creative and directive purpose,' mixing them, as it were, with 'a vital principle' so as to form living things, just as the painter might pick out certain colors from his confused palette and paint a picture.

This conception of the intermittent action of creative power and purpose does not, I confess, commend itself to me. That, however, is not so surprising as that it should be thought that this curious conception of the action of creative power is of value to religion. Whether the intermittent theory is a true or an erroneous conception seems to me to have nothing to do with 'religion' in the large sense of that word so often misused. It seems to me to be a kind of mythology, and, I should have thought, could be of no special assistance to teachers of Christianity. Such theories of divided creative operations are traceable historically to polytheism.

Lastly, with reference to Lord Kelvin's statement that "modern biologists are coming once more to a firm acceptance of something—and that is 'a vital principle.' " I will not venture to doubt that Lord Kelvin has such persons among his acquaintance. On the other hand, I feel some confidence in stating that a more extensive acquaintance with modern biologists would have led Lord Kelvin to perceive that those whom he cites are but a trifling percentage of the whole. I do not myself know of any one of admitted leadership among modern biologists who is showing signs of 'coming to a belief in the existence of a vital principle.'

Biologists were, not many years ago, so terribly hampered by these hypothetical entities—'vitality,' 'vital spirits,' 'anima animans,' 'archetypes,' 'vis medicatrix,' 'providential artifice,' and others which I can not now enumerate—that they are very shy of setting any of them up again.

Physicists, on the other hand, seem to have got on very well with their problematic entities, their 'atoms' and 'ether,' and 'the sorting demon of Maxwell.' Hence, perhaps, Lord Kelvin offers to us, with a light heart, the hypothesis of 'a vital principle' to smooth over some of our admitted difficulties. On the other hand, we biologists, knowing the paralyzing influence of such hypotheses in the past, are as unwilling to have anything to do with 'a vital principle,' even though Lord Kelvin erroneously thinks we are coming to it, as we are to accept other strange 'entities' pressed upon us by other physicists of a modern and singularly adventurous type. Modern biologists (I am glad to be able to affirm) do not accept the hypothesis of 'telepathy' advocated by Sir Oliver Lodge, nor that of the intrusions of disembodied spirits pressed upon them by others of the same school.

We biologists take no stock in these mysterious entities. We think it a more hopeful method to be patient and to seek by observation of, and experiment with, the phenomena of growth and development to trace the evolution of life and of living things without the facile and sterile hypothesis of 'a vital principle.' Similarly, we seek by the study of cerebral disease to trace the genesis of the phenomena which are supposed by some physicists who have strayed into biological fields to justify them in announcing the 'discovery' of 'telepathy' and a belief in ghosts.

Yours faithfully,

E. RAY LANKESTER.

LONDON, May 15, 1903.

I felt sure that I could not keep out of this interesting correspondence much longer, but I will try to be brief; and in congratulating Professor Ray Lankester on his admirable letter I should like to explain that the adjective 'fortuitous' as employed

by Lord Kelvin was evidently not selected by him as specially appropriate or illuminating, but merely used as part of a well-known phrase or quotation. It is clear that what our chief meant was that the formation of a crystal, and such like, proceeded in accordance with the unsupplemented laws of ordinary mechanics; whereas the formation of an animal or plant seemed controlled by something additional—viz., the presence of a guiding principle or life-germ, the nature of which neither I nor any other physicist in the least understands. I shall be surprised if biologists claim that they really understand it either.

It is true that Lord Kelvin employed the popular phrase 'creative power'—a phrase I should not myself use, because I am unable to define it—and in other respects his wording was more appropriate to a speech than to a philosophic essay, but nevertheless his speech as reported had all the usual subjective interest attaching to the freely-spoken personal convictions of a great man, attained as the outcome of a lifelong study of various aspects of nature.

As to the little parting shot at me about 'telepathy,' it is true that I regard it as a recently discovered fact, opening a new and obscure chapter in science; it is also true that Lord Kelvin, Professor Ray Lankester and nearly all biologists disagree contemptuously with this opinion. Well, we shall see. *Die Zeit ist unendlich lang.*

Yours faithfully,

OLIVER LODGE.

THE UNIVERSITY OF BIRMINGHAM,  
May 19, 1903.

#### SCIENTIFIC BOOKS.

*Reports of the Princeton University Expeditions to Patagonia, 1896-1899; I.—Narrative and Geography.* By J. B. HATCHER. Princeton, The University. 1903. 4to. Pp. xvi + 314; plates and map.  
From the rather meager remains of verte-

brates collected on the renowned voyage of the *Beagle* and turned over to Richard Owen by Darwin for study, paleontologists were first made aware of what has proved to be practically a new world of animal life which, though for the most part now extinct, was, within times geologically recent, extremely rich.

The novelty and wealth of this extinct fauna were fairly indicated by the discoveries of Fitzroy and Darwin, but the interest then aroused went little further until about 1887, when Señor Carlos Ameghino accompanied an expedition to southern Patagonia and began that series of discoveries which has since made him, and his brother Florentino, famous. The new world brought to light by them was totally unlike anything previously known among vertebrate faunas either living or fossil, and aroused the interest of paleontologists, geologists and zoologists everywhere.

Incidentally to the work of describing and classifying these remarkable remains certain hypotheses were advanced by the brothers Ameghino which concerned the relations of these fossil animals to those of the northern hemisphere, and the age assigned to the strata in which the fossils were found. These hypotheses were not generally accepted, and for some time it has been regarded as most desirable that an examination of the geology should be made by experts trained in other fields. This it was thought would harmonize the conditions revealed by observation in Patagonia with the results of expert work elsewhere, and clear up the confusion which seemed to have arisen in regard to the age and succession of the Patagonian strata.

It was for this purpose that Mr. Hatcher organized and carried out the explorations described in this volume, while he was curator of vertebrate paleontology for the university. Their primary object was to make observations and collections bearing on the geology and paleontology of the region, while such attention as circumstances allowed was from time to time directed to other branches of natural history. The cordial and effective cooperation of Professor W. B. Scott, head of the department of geology and paleontology,